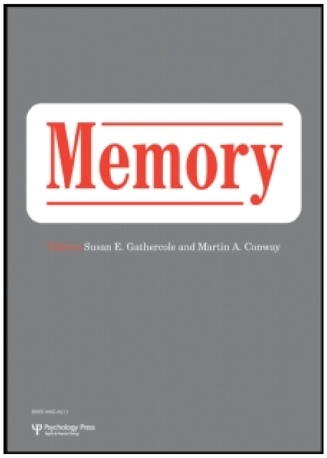

Memory
Publication details, including instructions for authors and subscription information:
http://www.informaworld.com/smpp/title~content=t713683358

On the costs of refocusing items in working memory: A matter of inhibition or decay?

Markus Janczyk [a]; Carolin Wienrich [b]; Wilfried Kunde [a]
[a] Dortmund University of Technology, Germany
[b] Martin Luther University Halle-Wittenberg, Halle, Germany

First Published on: 30 March 2008
To cite this Article: Janczyk, Markus, Wienrich, Carolin and Kunde, Wilfried (2008) 'On the costs of refocusing items in working memory: A matter of inhibition or decay?', Memory, 16:4, 374 - 385
To link to this article: DOI: 10.1080/09658210801941742
URL: http://dx.doi.org/10.1080/09658210801941742

MEMORY, 2008, 16 (4), 374–385

Psychology Press
Taylor & Francis Group

On the costs of refocusing items in working memory: A matter of inhibition or decay?

Markus Janczyk
Dortmund University of Technology, Germany

Carolin Wienrich
Martin Luther University Halle-Wittenberg, Halle, Germany

Wilfried Kunde
Dortmund University of Technology, Germany

The present study investigates the mental processes that are applied to previously attended items of working memory. In an object-switching task, participants counted the number of sequentially presented objects. In Experiment 1 the processing time increased when the object category switched from the prior trial compared to a repetition. More importantly, the further in the past the last instance of a current category was presented, the more processing time was necessary—an observation suggesting passive decay rather than inhibition of previously attended items. However, results differed when only two object categories were employed. Experiment 2 suggests that the lack of a clear indication of decay with small numbers of categories was due to participants' expectancy of category switches rather than repetitions. Taken together, the results suggest that working memory items become less accessible the longer they have not been attended to, when strategic processes are controlled.

An important characteristic of human cognition is the capability to briefly maintain information and allow cognitive operations to manipulate and alter this information. This capability has been conceptualised as ''working memory'' (Baddeley, 1986; Oberauer, 2002). To understanding such a system is of interest from a theoretical perspective and for various more applied purposes such as development and education (e.g., Gathercole, 1999) or cognitive ageing (e.g., Hasher & Zacks, 1988). Here we deal with one important aspect of working memory, namely switching attention between different contents of working memory. Specifically, we investigated the processes that occur once attention is released from (previously attended) items. First we will outline the framework that prompted the present research, and an experimental task widely used in this field. We will then describe two views on the destiny of working memory items from which attention has been released, and report two experiments that may help to clarify the appropriateness of these views.

An influential framework of working memory has been proposed by Cowan (1988, 1997, 2005), highlighting the interaction between (working) memory and attention. In this model, an activated subset of long-term memory representations constitutes working memory. Cowan called a subset of these activated representations (about four

Address correspondence to: Markus Janczyk, Dortmund University of Technology, Department of Psychology, Emil-Figge -Straße 50, 44227 Dortmund, Germany. E-mail: janczyk@fk14.uni-dortmund.de

We thank two anonymous reviewers for helpful comments on an earlier draft of this manuscript, and Sebastian Krüger for technical support. Parts of this work were supported by a grant of the German Research Foundation (DFG) to Wilfried Kunde.

http://www.psypress.com/memory DOI:10.1080/09658210801941742

items; Cowan, 2005) the ‘‘focus of attention’’, and cognitive processes are assumed to have access to this subset. Yet some results suggest that only one item at a time can be selected for manipulation through cognitive operations (e.g., Garavan, 1998; McElree & Dosher, 1989). To resolve these contradicting interpretations, Oberauer (2002) argued that both interpretations refer to different states of accessibility. Hence, he dissected Cowan’s focus of attention into two parts: the capacity-limited ‘‘region of direct access’’, and the ‘‘focus of attention’’, which holds only the one item involved in the next cognitive operation. The region of direct access holds a limited number of items that can be directly retrieved by the focus of attention. Activated items outside the region of direct access cannot be focused directly, and therefore do not interfere with processing the focused item.

Further support for the ‘‘1 item’’ view has been obtained with so-called *object-switching tasks* (or counter update tasks; Garavan, 1998) where participants are asked to count separately how often successively presented objects (e.g., squares or triangles) occur. Typically, reaction times (RT) are longer, when the category switches from the $(n-1)$th trial to the nth trial compared to when the category repeats. Interpreted in Oberauer’s (2002) framework, the currently updated counter is held in the focus of attention, while the other counter is memorised in the region of direct access. Upon a category switch, the focus of attention needs to retrieve the other counter. These cognitive costs yield the longer RTs, although this switch-repetition difference might reflect a small contribution of perceptual priming in repetition trials as well (Gehring, Bryck, Jonides, Albin, & Badre, 2003; Li et al., 2006).

But what exactly happens when the focus of attention is oriented towards another, not yet selected, item within the region of direct access? Different views on how such item switching is achieved exist. The *activation only model* assumes that switching the focus of attention raises the to-be-focused item’s activation to a certain level, whereas the activation of the no-longer-focused item simply decays over time to baseline. Alternatively, the *inhibition model* assumes that increasing the activation of the to-be-focused item is accompanied by an inhibitory process that reduces the activation of the abandoned item below baseline. Its activation then recovers to a normal level over time. Such inhibitory processes are widely assumed in task switching, and backward inhibition (BI) of previously relevant task sets appears to be an important component of task-set selection. To illustrate this, consider a study by Mayr and Keele (2000). In this study participants performed three different tasks (A, B, C) in succession. Most notably, RTs were longer for the last task in a series like ABA (inhibition sequence) than in a CBA series (control sequence). These ‘‘$n-2$ repetition costs’’ are assumed to occur because in the inhibition sequence task A is still in an inhibited state, thus requiring additional time to overcome this. While these costs are independent of the task preparation time (Hübner, Dreisbach, Haider, & Kluwe, 2003; Mayr & Keele, 2000; Philipp & Koch, 2006), they depend, for example, on the presence of task repetitions in sequences of trials (Philipp & Koch 2006). Similarly, BI is eliminated when spatial locations cue the forthcoming task (Arbuthnott, 2005). Thus it seems that the two means to accomplish task switching (activating the new task and inhibiting the old task) can be applied in varying ratios depending on certain (experimental) factors.

Whether a similar process is involved in item switching within working memory is an open issue that requires clarification. If the analogy holds, BI should temporarily suppress/inhibit the recollection of recently abandoned items (e.g., by reducing the abandoned item’s activation to below baseline). Thus each item’s residual activation (and therefore its potential to be selected by the focus of attention) would be the sum of its remaining activation and the opposed inhibition. Such interplay was already posited to explain the error-priming effect in mathematical cognition (Campbell & Clark, 1989) and the control of sequential retrieval (Arbuthnott, 1996). We assume that this inhibition (if existing) decays over time (Campbell & Clark, 1989; but see Gade & Koch, 2005, for a different view on this in task switching).

Based on these assumptions, both models make different predictions regarding the recollection of items from which attention has been released. (1) Assuming the ‘‘activation only model’’, a recently focused item is more *easily* (i.e., more rapidly) recollected when only little time has elapsed from focusing it. (2) According to the ‘‘inhibition model’’, the less time has elapsed, the less the activation has recovered from the inhibition, and thus an item is *harder* to retrieve with only little time after its last recollection.

There have already been attempts to discriminate between these possibilities. Oberauer (2003) used memory updating tasks where participants applied operations ranging from "subtracting 8" to "adding 8" (excluding "0") to one to four counters (set size 1–4). He then analysed lag effects, with lag meaning the number of different counters between two recollections of a specific counter (e.g., ABCDA would be lag 3 with set size 4). Overall, lag 1 RTs were reliably shorter than those for higher lags and there was no sign of decreasing RTs with increasing lag, thus no trace of BI. Yet, as adding "1" is conceivably easier than subtracting "8", the lag analyses might have been affected by task difficulty inadvertently. The same overall result was reported from a study employing a modified *n*-back task (Oberauer, 2006).

However, Bao, Li, Chen, and Zhang (2006) reported contradicting evidence using a counter-switching task with three different object categories (requiring participants to maintain three counters simultaneously). Comparing inhibition sequences (ABA) with control sequences (CBA) they found longer RTs to the last items of inhibition sequences compared to the last items of a control sequences ("$n-2$ repetition costs"). This is more consistent with the inhibition model. Bao et al. (2006) suggested that the counters in their study were memorised in the region of direct access, whereas in Oberauer's (2003) experiments they might have been held in the activated long-term memory. Yet the significant set-size effect in the latter study suggests that the counters were memorised in the capacity-limited region of direct access. Bao et al. (2006, p. 217) were also aware that expectancies regarding the next item can explain their results. Specifically, they noted that the sequential expectancy effect might have been responsible for the results (p. 217). That means that after having seen, for example, the items A and B, participants tend to expect a C rather than an A again. Thus upon the surprising re-occurrence of an A, any preparation for a C needs to be overcome, and this can also account for the "$n-2$ repetition costs". To rule out these expectancies Bao et al. (2006) used explicit cues. In Experiment 3, participants started with three counters, each associated with a specific colour and the starting value 5. In each trial a coloured frame indicated to which counter the forthcoming operation should be applied. Then either a "+" ("add 1") or a "−" ("subtract 1") appeared, and participants applied this operation to the cued counter. Bao et al. (2006) still reported "$n-2$ repetition costs", arguing that due to the counter cueing, expectancies were ruled out.

A disadvantage of this approach, however, is that the effect of expectancy might actually not have been ruled out since participants may not have used these item cues. For example, it was suggested that in a considerable portion of cases participants fail to use task cues for task preparation ("failure to engage" hypothesis in task switching; De Jong, 2000; Nieuwenhuis & Monsell, 2002; but see Lien, Ruthruff, Remington, & Johnston, 2005).

Here we present two experiments that aimed at clarifying the above-mentioned contradiction, suggesting and demonstrating that expectancies do indeed affect RTs in object-switching tasks and thus may explain Bao et al.'s results (2006). Every sequence of categories (in an object-switching task) can be described as a sequence of two salient events: category repetitions (the same as the prior category) and category switches (other than the prior category). In such sequences participants often expect more switches than repetitions, a phenomenon described as the "gambler's fallacy" (Soetens, 1998; Waagenar, 1972). Conceivably, this switch expectancy increases with consecutive repetitions. Importantly, with small numbers of possible categories a rather specific category can be expected to appear upon a switch. In particular, when only two categories are used, an expectancy of a switch means to expect a specific category (the currently unused one). Via top-down modulation, participants might then engage in specific preparation to switch to a specific counter with runs of repetitions, e.g., by raising the expected counter's activation beforehand and facilitating its subsequent retrieval into the focus of attention. Consequently, switch expectancies would reduce RTs to switches as repetitions increase when only two categories are used, and thus mimic a data pattern consistent with the inhibition model (higher RTs for recently seen categories). Such a pattern was reported by Gehring et al. (2003, Figure 7b).

Although it might be that participants use or do not use BI in strategic ways depending on the number of counters, this would not be a parsimonious explanation. Instead we pursued the simpler idea that BI (as outlined above) is *not* involved in item switching within working memory, and argue that item expectancies indeed affect performance in object-switching tasks and might even produce RT patterns that could falsely be interpreted as a product of BI.

To test this idea we used an object-switching task in Experiment 1, with orders of categories where the number of categories varied (2 or 4), but the probability of switches was identical ($p = .5$). To anticipate the main results, we observed no evidence for the inhibition model with larger numbers of categories (4) where switch expectancies were not associated with a specific alternative. In contrast, evidence for the inhibition model was observed as predicted with a small number of categories (2) where switch expectancy was related to a specific category identity. In Experiment 2 the signs of inhibition vanished when the participants' expectancies were equated between small and large numbers of categories.

EXPERIMENT 1

In Experiment 1 participants worked on an object-switching task (e.g., Garavan, 1998). The purpose of this experiment was to demonstrate that expectancies regarding the next upcoming item category can affect performance in such tasks. Therefore we used conditions in which participants had to update either two or four counters. These numbers were chosen to maximise the difference between conditions while keeping the task feasible for participants. In contrast to Oberauer (2003) we chose the Garavan task to keep the operation constant ("add 1"). Of particular interest here is whether RTs at lag 1 (e.g., ABA) are shorter or longer than those at higher lags (e.g., ABBA or ABCBA). If longer, this would support the inhibition model outlined in the introduction. Otherwise the activation only model would remain viable. Based on the above-mentioned logic we predict BI compatible patterns for the set size 2 condition but not for set size 4.

Necessarily, both conditions differed in another aspect. In set size 2, between the two recollections of one category, only repetition items can occur (with lags higher than 1). In contrast, in set size 4 switches between other categories can also occur. If the number of switches (or the number of different intervening categories: Oberauer, 2003, p. 262) between the recollections of one category is the crucial factor, we would expect different RTs to the last item in runs like ABBCA (one intervening switch) and ABCBA (two intervening switches). We return to this point in the results section.

Method

Participants. A total of 15 students (14 female) from Martin Luther University Halle-Wittenberg, with a mean age of 21.1 years, participated in this experiment. Participants were naïve regarding the purpose of this experiment and received course credit in return. Due to a computer malfunction we lost the data from one participant. Additionally, two participants did not fulfil our accuracy criterion (see below). Thus, data are reported for 12 participants.

Design. We used a counter-switching task (e.g., Garavan, 1998) where each trial made use of either all four (set size 4) or of two (set size 2) of four possible categories (triangle, rectangle, circle, diamond). The task was to update counters of how many items of these different categories occurred during a trial. All participants performed two sessions of four unanalysed practice trials and 48 test trials. The test trials were arranged in four blocks of 12 trials, separated by a forced break of 45 seconds. Each block comprised six set size 4 and six set size 2 (one of each possible combination of two objects) trials. (For details on trial construction, see below.) A trial comprised the successive presentation of 20–25 items one at a time. The variation was necessary to hinder participants from calculating the count of a category by means of subtraction from a constant total.

Each trial began with the presentation of the (two or four) relevant categories and was started by pressing the spacebar. After that, items of the current trial were presented one at a time on the computer screen. Progression within a trial was self-paced, i.e., pressing the spacebar cleared the

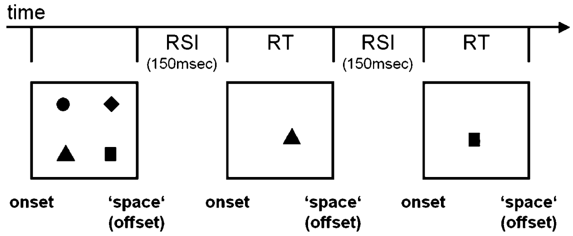

Figure 1. Illustration of a trial run: Participants were presented on the first screen with the categories relevant to be counted (here, a set size 4 example). Items were then presented one at a time. Each item disappeared when the participant pressed the spacebar. (Figure not drawn to scale.)

screen and displayed the next object with a response–stimulus interval of 150 ms (see Figure 1). Pressing the spacebar was used to measure reaction times (RT).[1] At the end of each trial the screen went blank and participants reported their counts of each category on a prepared sheet of paper. Pressing "enter" started the next trial. Items were not presented centrally, but their location was randomly chosen within an imaginary rectangle one-half of the screen size, centred on screen, to avoid the occurrence of two equal shapes at the same position in repetition trials.

Trials. Each trial's first item was chosen randomly. Then, the probability of a category switch was $p = .5$. For set size 2, after three repetitions a switch was forced. For set size 4 this happened after two repetitions, and upon a switch the next category was chosen out of the three remaining categories with equal probability. This pseudo-random sequence slightly favoured patterns of putative inhibition more in set size 4 than in set size 2: If participants were able to "recognise" the algorithm implicitly, this was likely to happen earlier or more frequently in set size 4 trials where participants could predict a switch after only two repetitions. Thus, if anything, the employed pseudo-random sequence created a stronger bias towards a data pattern consistent with inhibition for set size 4 than for the set size 2 trials, and thus worked against our predictions.

Procedure. Participants were tested individually in an experimental room at Martin Luther University Halle-Wittenberg. Each session lasted about 45 minutes, with the second session scheduled roughly 1 week after the first. We used different trials for both sessions, but the same trials across all participants. Participants were given written instructions, emphasising the importance of accuracy while proceeding as fast as possible. Participants were told that the categories would appear in a random order. For the second session participants were not given instructions again, except for a reminder to be fast and accurate. Items were black on a white background in this experiment, presented via a Siemens notebook on a 31 × 23 cm screen.

Analyses. A trial was judged as correct and included for RT analyses only if at least half of the categories were counted correctly. Additionally, we only used data from participants with at least 50% correct trials. Two participants did not fulfil this criterion and their data were discarded. RTs lower than 300 ms and RTs exceeding an individual's mean by more than 2.5 individual standard deviations were excluded. Since the first item can neither be a repetition nor a switch, those RTs were also excluded from analyses. Data were categorised (1) as switch of a category (SC) or non-switch (NSC) and (2) as their corresponding lag position (e.g., lag 1: ABA; lag 2: ABCA; lag 3: ABCBA). "Set size" (2 vs 4), "switch" (NSC vs SC), and "lag" (1–3) were treated as within-participant factors. The alpha error level was set to $p < .05$.

Results

Error analysis. Mean error percentage was 7.21 ($SD = 6.52$) and counting errors were not systematically associated with the number of items in a trial [$\chi^2(5) = 5.81$, $p = .33$]. In more than 80% of set size 2 and more than 50% of set size 4 trials participants reported the correct values for all counters (see Table 1). Graphical inspection of the deviation (reported minus correct values) revealed a symmetrical distribution for both set size conditions, ranging from -5 to $+6$ (set size 2) and from -8 to $+6$ (set size 4). Hence, we believe that participants were diligent in updating the counters.

RT analyses. Individual mean RTs (see Table 2) were submitted to a repeated measures analysis of variance (RM ANOVA) with factors set size and switch. This revealed significant main effects of set size [$F(1, 11) = 99.05$, $p < .01$, partial $\eta^2 = .90$], and switch [$F(1, 11) = 61.28$, $p < .01$, partial $\eta^2 = .85$]. The interaction was not significant [$F(1, 11) = 3.03$, $p = .11$, partial $\eta^2 = .22$]. Participants were faster on NSC items than on SC items, and they were faster in set size 2 trials than in set size 4 trials. Set

[1] As a consequence, the interval between items (ITI) varied with RT. One could argue that with long RTs (long ITI) the decay of previously focused items would be more progressed than with short RTs, which consequently requires less inhibition of the previous item (see Gade & Koch, 2005, for this idea in task switching). Yet keeping the ITI constant would mean to vary the response–stimulus interval (RSI = ITI – RT). This would open the door for other uncontrollable effects. Participants might simply press the spacebar once they perceptually recognised the current category, and update the counter during the (rather long) RSI. Also this would allow more preparation for the next item the shorter the RT in the current trial, obviously compromising the interpretation of the RT measure as well. We therefore opted to keep procedural details of the experiment as close as possible to previously published work on item switching (Bao et al., 2006; Oberauer, 2003).

TABLE 1
Counting errors

	Experiment 1	*Experiment 2*
Set size 2		
2 count error	8.6	1.7
1 count error	8.9	7.8
all correct	82.5	90.5
Set size 4		
4 count error	4.9	2.5
3 count error	19.5	4.3
2 count error	15.1	14.3
1 count error	7.3	23.4
all correct	56.2	55.5

Percentage of counting errors made during both experiments (e.g., ''2 count error'' meaning that the values of two counters in a given trial differed from the correct value).

size also affected repetition items, with longer RTs for set size 4 than for set size 2 trials. Next, mean RTs were submitted to an RM ANOVA with factors set size and lag. On lag we computed Helmert contrasts. None of the two Helmert contrasts was significant [lag 1 vs lag 2–3: $F(1, 11) < 1$, partial $\eta^2 = .03$; lag 2 vs 3: $F(1, 11) = 3.20$, $p = .10$, partial $\eta^2 = .23$], but both interacted with set size [lag 1 vs lag 2–3: $F(1, 11) = 34.10$, $p < .01$, partial $\eta^2 = .76$; lag 2 vs 3: $F(1, 11) = 5.81$, $p < .05$, partial $\eta^2 = .35$]. Figure 2 (left panel) illustrates this interaction. While the plotted data for set size 4 speak in favour of the ''activation only model'', the data for set size 2 are indeed consistent with the ''inhibition model''. Separate RM ANOVAs for both set size conditions with factor lag confirmed this. For set size 2 the first Helmert contrast was significant; the second was not [lag 1 vs lag 2–3: $F(1, 11) = 7.85$, $p < .05$, partial $\eta^2 = .42$; lag 2 vs lag 3: $F(1, 11) = 1.56$, $p = .24$, partial $\eta^2 = .12$]. Thus RTs for lag 1 were indeed longer than those for higher lags. For set size 4 both Helmert contrasts were significant [lag 1 vs lag 2–3: $F(1, 11) = 11.35$, $p < .01$, partial $\eta^2 = .51$; lag 2 vs lag 3: $F(1,11) = 7.51$, $p < .05$, partial $\eta^2 = .41$] in a way predicted from the ''activation only model''.

We also categorised lag 2 and lag 3 RTs from set size 4 trials with respect to the number of switches between the two recollections of the critical category—lag 2: no switches (ABBA) vs one switch (ABCA); lag 3: one switch (ABBCA) vs two switches (ABCBA). RTs to the last items of such runs were submitted to two separate RM ANOVAs with ''number of intervening switches'' as a within-participant factor. Both analyses revealed no significant effect [lag 2: $F(1, 11) < 1$, partial $\eta^2 = .006$; lag 3: $F(1, 11) < 1$, partial $\eta^2 = .004$]. This indicates that performance was not affected by the number of intervening category switches. (But note that the sample of RTs was

TABLE 2
Mean RTs and averaged item positions

	Experiment 1		*Experiment 2*	
	RT [*M* (*SD*)]	*AIP* [*M* (*SD*)]	*RT* [*M* (*SD*)]	*AIP* [*M* (*SD*)]
Set size 2				
no-switch	918.27 (144.87)		834.36 (157.47)	
switch	1367.52 (329.72)		1212.76 (330.77)	
lag 1	1447.50 (335.45) [1352.51]	14.35 (6.06)	1199.62 (293.75) [1101.47]	13.61 (6.47)
lag 2	1376.31 (356.77) [1305.55]	13.09 (5.73)	1281.21 (293.98) [1253.33]	12.89 (5.77)
lag 3	1350.76 (372.68) [1293.50]	13.59 (5.32)	1260.37 (333.25) [1172.72]	15.64 (5.40)
Set size 4				
no-switch	1547.24 (334.48)		1710.52 (487.24)	
switch	2064.20 (485.38)		2322.14 (632.82)	
lag 1	2012.96 (466.37) [1938.10]	12.87 (6.15)	2339.55 (661.61) [2229.08]	13.33 (6.12)
lag 2	2094.50 (497.49) [2005.92]	13.51 (6.23)	2399.42 (677.14) [2225.80]	14.13 (5.57)
lag 3	2162.45 (519.25) [2053.26]	13.35 (5.32)	2457.91 (762.02) [2460.77]	13.32 (5.91)

Mean RTs in milliseconds (RT) and averaged (lag 1–3) item positions (AIP) within trials for Experiments 1 and 2. (Numbers in box brackets represent corrected RTs resulting from including ''position set'' as a covariate.)

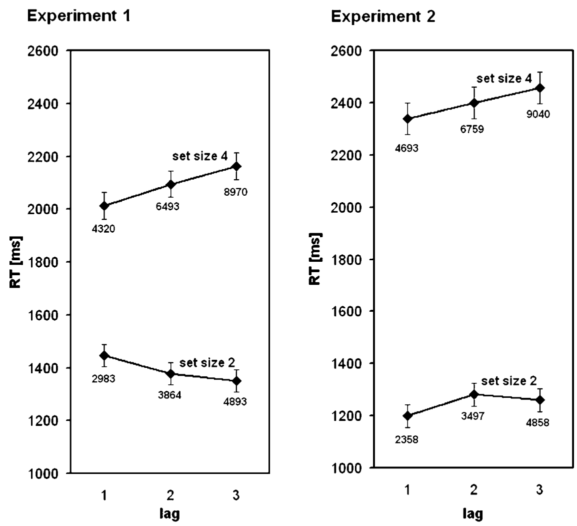

Figure 2. RTs of lag 1, 2, and 3 items for both set size conditions in Experiment 1 (left panel) and 2 (right panel). The numbers appearing at the data points are the actual mean delays since the most recent onset of the current category (in milliseconds). The bars represent the 95% within-participant confidence interval within each condition.

rather small varying between $n = 11$ and $n = 103$, depending on participant and condition.)

Finally we computed the mean RTs for four sets of five successive items in a trial (with items on positions 16 to 25 summarised in the fourth set). This indicated growing mean RTs with increasing item position (Figure 3; see Gehring et al., 2003)—what we refer as to the ‘‘item position effect’’.

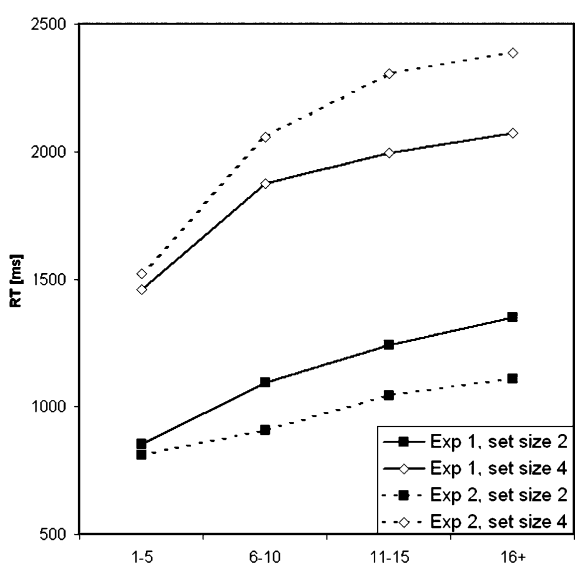

Figure 3. RTs as a function of item position within a trial.

Discussion

Experiment 1 replicated several findings from the working memory literature: RTs were longer to switches than to repetitions of items, and performance was also superior with two rather than four updated counters. Interestingly, we did not observe significantly larger switch costs with set size 4 compared to set size 2. To anticipate, in Experiment 2 we observed a significant interaction and will discuss this in the General Discussion. Also we replicated the item position effect, i.e., growing RTs with increasing item positions (Gehring et al., 2003). Overall the typical findings were replicated.

Our main question concerned the lag analyses. For set size 4 the data argue for the ‘‘activation only model’’. In contrast, for set size 2 the data are compatible with the ‘‘inhibition model’’. This was predicted from the hypothesis that switch expectancies (a) increase with the numbers of repetitions encountered before, and (b) are related to specific category identities for conditions with small set sizes.

A possible alternative explanation can be derived from the effect of stimulus sequence (Gehring et al., 2003). With set size 2, the number of repetitions (of the other category) amounts to lag minus 1 (i.e., lag $2 = 1$ repetition; lag $3 = 2$ repetitions). The results of Gehring et al. (2003) suggest that the number of uninterrupted repetitions before a switch speeds up the reaction on the following switch (Figure 7b of that study). This data pattern nicely corroborates our own observation and might also have been driven by participants’ expectancies of a switch following runs of repetitions. One remaining question is why such expectancies apparently do not increase RTs in a sequence of repetitions (i.e., when the current item is a repetition as well, Figure 7a of that study). Here we agree with Gehring et al. (2003, p. 573) who assume that bottom-up processes (repetition priming) and top-down processes (gambler’s fallacy) can coexist, with one impact being stronger than the other depending on experimental conditions. Thus decreasing RTs to repetitions after runs of repetitions do not exclude expectancies affecting performance as well, although not to an extent that exceeded repetition priming effects. One might expect repetition priming to be acting across intervening items, and thus the last reaction to an ABA run to be faster than to an ABBA run. This is opposite to what we found in the set size 2 condition of Experiment 1, where ABA sequences correspond to lag 1 items and ABBA sequences correspond to lag 2 items. Yet a careful examinations of Gehring et al.'s data (2003, their Figure 7b) suggests that repetition priming does not consistently operate across intervening items. For example, the last response to a 2112 sequence was slightly *faster* than to a 212 sequence, similar to what we found. Moreover, reactions to 1212 sequences were slower than to 2112 runs, although in the former case the priming effect should actually be stronger, assuming it acts across intervening trials (i.e., switches), and the same is the case, e.g., for reactions to runs like 12112, which are slower than the last reactions to 21112 runs. The reasons for the loss of the facilitating impact of repetitions when an item switch occurs have to be further explored. It might be that a speed-up due to cumulative activation of a task representation occurs only as long as the task is attended to, and drops immediately (is "flushed") as soon as another task is encountered.[2]

EXPERIMENT 2

The results of Experiment 1 are generally consistent with the idea that expectancies do indeed affect performance in object-switching tasks and can even mimic data patterns consistent with the inhibition of previously attended items. If this hypothesis is correct, it should be possible to remove these signs of putative inhibition. One way to do so is to remove participants' implicit expectancies by using explicit category cues (see Bao et al., 2006). Driven by our reservations concerning this (see Introduction), we used a different approach in Experiment 2. While the overall design remained the same as in Experiment 1, we equated participants' expectancies about the next item across conditions by using all four categories in all trials. However, participants had to count only two of them in set size 2 trials. If different expectancies could account for the findings of Experiment 1, we should now observe the same pattern across both set size conditions. From our theoretical assumptions we predicted a pattern favouring the "activation only model" across both set size conditions (i.e., shorter RTs to lag 1 items than to higher lags).

[2] We thank a reviewer of the manuscript for this suggestion.

Method

A total of 18 students (15 female) from Martin Luther University Halle-Wittenberg, with a mean age of 21.1 years, received monetary compensation for their participation. One participant (female) did not attend the second session; another participant (male) was recovering from a serious head injury, which we learned of only after conducting the experiment. Both participants' data were excluded from the analyses.

The general method and procedure were the same as in Experiment 1. While set size 4 trials were not altered, set size 2 trials were changed in the following way: Participants were to count only two categories (indicated before the beginning of each trial), but all four categories appeared during the trial. At the end of each trial, participants were prompted to enter their counts of the categories. Participants were instructed to proceed (by pressing the spacebar) as quickly as possible upon the identification of an irrelevant item. Two sets of trials were constructed according to the method described above for set size 4 and order of these sets was counterbalanced across participants. Stimuli were presented on a 33×24 cm screen via a PC. Since in set size 2 not every item was relevant, some changes for the data analyses were necessary and we included only those items that were actually relevant to be counted in a trial. For analysing switch costs we included the *n*th item only if the $(n-1)$th item was also relevant in this trial; for the lag analysis we included an item only if between the current and the last recollection at least one other relevant item occurred, thus making a switch of the focus of attention necessary.

Results

Error analyses. Mean error percentage was 4.22 ($SD = 3.59$). The occurrence of counting errors was associated with the number of items in a trial [$\chi^2(5) = 13.94$, $p < .05$]. In general, the percentage of errors increased with increasing number of items in a trial. As in Experiment 1 in more than 80% of set size 2 and more than 50% of set size 4 trials participants reported the correct values for all counters (see Table 1). Graphical inspection of the deviation again revealed a symmetrical distribution for both set size conditions, ranging from -3 to $+5$ (set size 2) and from -7 to 5 (set size 4). Again, we believe that participants were diligent in updating the counters.

RT analyses. Individual mean RTs (see Table 2) were submitted to a RM ANOVA with factors set size and switch. The main effects of set size [$F(1, 15) = 93.19$, $p < .01$, partial $\eta^2 = .86$], and switch [$F(1, 15) = 130.03$, $p < .01$, partial $\eta^2 = .90$], were significant, as was the interaction [$F(1, 15) = 20.45$, $p < .01$, partial $\eta^2 = .58$]. Participants were faster on NSC items than on SC items and were also faster in set size 2 trials than in set size 4 trials. Switch costs were greater for set size 4 than for set size 2. Also, RTs to repetition items were longer for set size 4 than for set size 2. Mean RTs were then submitted to a RM ANOVA with factors set size and lag. On lag we computed Helmert contrasts. The first Helmert contrast was significant [lag 1 vs lag 2–3: $F(1, 15) = 14.00$, $p < .01$, partial $\eta^2 = .48$], with shorter RTs to lag 1 items than to those of higher lags. The second Helmert contrast was not significant [lag 2 vs 3: $F(1, 15) < 1$, $p = .47$, partial $\eta^2 = .04$]. No Helmert contrast interacted with set size [lag 1 vs lag 2–3: $F(1, 15) < 1$ $p = .74$, partial $\eta^2 = .01$; lag 2 vs 3: $F(1, 15) = 2.69$, $p = .12$, partial $\eta^2 = .12$] (see Figure 2, right panel).

Next, we computed the mean RTs for four sets of five successive items in a trial (with items on position 16 to 25 summarised in the fourth set), yielding the item-position effect (Figure 3).

A final analysis aimed at demonstrating switch costs for set size 2 with irrelevant items occurring between two relevant items. Thus in a trial where A and B would be relevant, the last item of a run like ACDA was categorised as NSC while ACDB would be SC. An RM ANOVA with switch as a within-participant factor yielded reliable switch costs [$M(\text{NSC}) = 1021$, $M(\text{SC}) = 1152$, $F(1, 15) = 23.66$, $p < .01$, partial $\eta^2 = .1$]. This supports that, even with intervening irrelevant stimuli, the focus of attention stayed on the last relevant item.

Discussion

As expected we observed effects of both factor set size and switch, and switch costs were higher the more counters were to be held in the region of direct access. Taken together, these results are in line with the working memory model advanced by Oberauer (2002).

Of particular interest here are the results of our lag analysis. Having controlled for different expectancies, RTs to lag 1 items were significantly shorter than those to higher lags across both set sizes. This pattern shows no sign of BI involved in item switching. One might argue that due to the irrelevant items introduced in set size 2 trials, the focus of attention did not stay on an item during the appearance of such items. Since we demonstrated reliable switch costs even in those cases, we assume that the focus of attention did indeed focus on the last relevant item, justifying the reported lag analysis.

Finally, the results of Experiment 2 rule out another alternative explanation of Experiment 1. One may argue that the results of Experiment 1 were due to the fact that only in set size 2 trials was the interference from the not-focused counter particularly large, thus requiring the largest amount of BI. Yet in Experiment 2 participants also updated only two counters (evident in the significant set size effect). Thus the amount of interference was comparable across both experiments, but results differed. So what seems crucial to obtain a data pattern that looks like BI is the expectancy of a certain stimulus identity rather than increased interference.

GENERAL DISCUSSION

The present experiments attempted to clarify whether backward inhibition (BI) is involved in item switching within working memory or not. BI is a well-established phenomenon in research on task switching (e.g., Hübner et al., 2003; Mayr, 2002; Mayr & Keele, 2000). Yet in item switching within working memory there are relatively few data, and then with contradicting results. While some data contradict BI in item switching (Oberauer 2003, 2006), other support BI (Bao et al., 2006). We suggest here that the latter results might result artificially from specific expectancies concerning the forthcoming item (see also Bao et al., 2006, p. 217). These authors used explicit cues to attenuate such expectancies, but the participants might not have used these cues (‘‘failure to engage’’ hypothesis; De Jong, 2000). Therefore we used a different approach to show that expectancies do indeed affect performance in object-switching tasks. We assume that (a) participants increasingly expect a switch of items with longer runs of repetitions (‘‘gambler's fallacy’’; Soetens, 1998; Waagenar; 1972), and (b) with fewer to-be-updated counters, there is a greater expectancy of a switch to a specific counter. With only two counters, this expectancy relates exclusively to the currently not-focused counter.

Experiment 1 aimed at demonstrating the expected differential effects for different amounts of counters. In accordance with our assumptions we observed a pattern compatible with BI (i.e., longer RTs to a category when less time has elapsed since its last recollection) with set size 2. In contrast, with set size 4 the data pattern spoke against BI, suggesting that upon a switch the activation of the last counter simply decays over time.

In Experiment 2 we determined whether these results were indeed caused by different expectancies between both set size conditions. Here, we equated participants' expectancies between both set sizes by presenting identical category sequences with all four categories in both conditions. However, in set size 2 trials participants had to count only two of them. As expected, we now observed a similar data pattern across both set size conditions, failing to reveal BI.

It should be made clear once again that Bao et al.'s (2006) and the present experiments are not totally comparable. Whereas our approach is based on performance differences at different lags, their approach is based on the comparison of inhibition sequences (e.g., ABA) with control sequences (CBA). Still, the authors suspect that expectancies might explain their results, namely the expectancy that a previously encountered item will not re-occur as quickly as it does in an ABA sequence. In other words, following an AB sequence a switch to a specific identity (C) is more expected than a switch to another identity (A). Thus, even with three counters, counter-specific expectancies might still operate when sequences of three events are considered. Although this possibility was dismissed on the grounds that cueing the next item does not eliminate the performance costs of inhibition sequences, cueing might not fully abolish such expectancies, as explained in the introduction. We therefore suggested our own approach, which admittedly comes at the cost that the two studies are not directly comparable at the experimental level.

Another important difference concerns the sequencing of stimuli. Whereas Bao et al. (2006) used a task sequence that was essentially random, the task sequence in the present experiments was somewhat constrained in that a switch had to occur after three (set size 2) or two (set size 4) repetitions. First of all, it is notably a data pattern consistent with the gambler's fallacy (faster responding with lag 2 than lag 1) even though constraints were introduced only at lag 3 trials (where a switch was forced). What is more, even if the gambler's fallacy was created artificially, we demonstrated that expectancies have the power to affect RTs in object-switching tasks and even to induce a BI-like data pattern (in experiments like ours). Thus, at the least the present results should alert researchers to take such expectancies into account, for whatever reason they occur. Whether or not similar expectancies were operating in the Bao et al. (2006) study is indeed a question for future research.

Several alternative explanations are worth considering. One can argue that our failure to find BI-consistent RT patterns for the set size 4 trials was due to longer actual times that passed since a category's last onset and BI might simply be decayed up to this point. To facilitate evaluation we added the actual mean delays for all data points in Figure 2. We like to point out one thing in this context: The actual mean delays for set size 2 trials were *longer* in Experiment 1 than in Experiment 2. Thus we would be expecting BI to be more decayed in Experiment 1 than in Experiment 2. Yet we found BI-consistent RT patterns in Experiment 1 but not in Experiment 2. At least for these critical findings full BI decay appears to be an unlikely explanation.

Additionally—as we assume both bottom-up repetition priming and top-down expectancy to be interwoven—one could argue that in large part we did not find patterns consistent with BI because repetition priming might simply have masked this. Even though this is possible in theory, such priming effects were certainly not powerful enough to overrun BI in comparable studies on task switching (e.g., Mayr & Keele, 2000) or counter switching (Bao et al., 2006; assuming that their results were actually due to BI). There is no a priori reason why priming should have been powerful enough in the present study.

As both our experiments exhibited growing RTs with increasing item position (the item-position effect; Gehring et al., 2003), there is another alternative interpretation of our critical results in Experiment 1: If lags 2 and 3 were more prevalent in early positions in set size 2 trials than were lag 1 items, this could also have produced the observed decrease in RTs. However, variation in average item position was minimal (see Table 2) and in many cases a pattern would be predicted opposite to our observations. Although there are some comparisons consistent with our reported RTs, more than half of the comparisons are not (five out of eight). Additional analyses, in which we categorised item positions into four sets of five successive items (items on position 16 to 25 summarised in the fourth set) and included "position set" as a covariate in the lag analyses, did not change the overall picture of RT patterns. Hence, an uneven distribution of positions across lags cannot explain the present results.

The item-position effect might also explain the lack of an interaction between set size and switch in Experiment 1. The counters' mean values in set size 2 trials were necessarily higher than those in set size 4 trials, and thus the RTs to switch items in set size 2 would be expected to be prolonged. In contrast, in Experiment 2 the mean counter values were comparable across both set size conditions, and here the interaction was observed as expected.

Another point is the influence of set size on repetition items: In both experiments RTs to repetition items were longer in set size 4 trials than in set size 2 trials. This might come as a surprise, as there should be no need to refocus a repetition item. Oberauer (2003) did not find such an effect in his Experiment 1. However, in his Experiment 2, RTs to repetition items increased from set size 2 (1150 ms) to set size 4 (1600 ms). His Experiment 1 required retrieval of items but no updating, whereas his Experiment 2 required updating but no retrieval. Since our task included retrieval *and* updating, possibly these results are due to more difficult updating with greater set sizes (instead of mere selection). Nevertheless, they are also consistent with a bigger focus of attention (Cowan, 1988, 1997, 2005) and more research is required to resolve this issue.

To conclude, we replicated findings compatible with the working memory model proposed by Oberauer (2002). Most importantly, the experiments lend support for the idea that item selection within working memory does not use BI to resolve interference, as has been observed in task switching (Hübner et al., 2003; Mayr, 2002; Mayr & Keele, 2000). Thus, inhibition might be seen as a process applied in many contexts, even though it seems not to support switches between items in working memory. Upon a switch of the focus of attention within working memory, activation of prior focused items appears to simply decay over time.

Manuscript received 17 September 2007
Manuscript accepted 24 January 2008
First published online 30 March 2008

REFERENCES

Arbuthnott, K. D. (1996). To repeat or not to repeat: Repetition facilitation and inhibition in sequential retrieval. *Journal of Experimental Psychology: General*, *125*, 261–283.

Arbuthnott, K. D. (2005). The influence of cue type on backward inhibition. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *31*, 1030–1042.

Baddeley, A. D. (1986). *Working memory*. Oxford, UK: Clarendon Press.

Bao, M., Li, Z-H., Chen, X-C., & Zhang, D-R. (2006). Backward inhibition in a task of switching attention within verbal working memory. *Brain Research Bulletin*, *69*, 214–221.

Campbell, J. I. D., & Clark, J. M. (1989). Time course of error priming in number-fact retrieval: Evidence of excitatory and inhibitory mechanisms. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *15*, 920–929.

Cowan, N. (1988). Evolving conceptions of memory storage, selective attention, and their mutual constraints within the human information processing system. *Psychological Bulletin*, *104*, 163–191.

Cowan, N. (1997). *Attention and memory: An integrated framework*. New York: Oxford University Press.

Cowan, N. (2005). *Working memory capacity*. Hove, UK: Psychology Press.

De Jong, R. (2000). An intention activation account of residual switch costs. In S. Monsell & J. Driver (Eds.), Control of cognitive processes. *Attention and performance XVIII* (pp. 357–376). Cambridge, MA: MIT Press.

Gade, M., & Koch, I. (2005). Linking inhibition to activation in the control of task sequences. *Psychonomic Bulletin & Review*, *12*, 530–534.

Garavan, H. (1998). Serial attention within working memory. *Memory and Cognition*, *26*, 263–276.

Gathercole, S. E. (1999). Cognitive approaches to the development of short-term memory. *Trends in Cognitive Sciences*, *3*, 410–419.

Gehring, W. J., Bryck, R. L., Jonides, J., Albin, R. L., & Badre, D. (2003). The mind's eye, looking inward? In search of executive control in attention shifting. *Psychophysiology*, *40*, 572–585.

Hasher, L., & Zacks, R. T. (1988). Working memory, comprehension, and aging. A review and a new view. In G. H. Bower (Ed.), *The psychology of learning and motivation, Vol. 22* (pp. 193–225). New York: Academic Press.

Hübner, M., Dreisbach, G., Haider, H., & Kluwe, R. H. (2003). Backward inhibition as a means of sequential task control: Evidence for reduction of task competition. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *29*, 289–297.

Li, Z., Bao, M., Chen, X., Zhang, D., Han, S., He, S., et al. (2006). Attention shift in human verbal working memory: Priming contribution and dynamic brain activation. *Brain Research*, *1078*, 131–142.

Lien, M-C., Ruthruff, E., Remington, R. W., & Johnston, J. C. (2005). On the limits of advance preparation for a task switch: Do people prepare all the tasks some of the time or some of the task all the time? *Journal of Experimental Psychology: Human Perception and Performance*, *31*, 299–315.

Mayr, U. (2002). Inhibition of action rules. *Psychonomic Bulletin & Review*, *9*, 93–99.

Mayr, U., & Keele, S. W. (2000). Changing internal constraints on action: The role of backward inhibition. *Journal of Experimental Psychology: General*, *129*, 4–26.

McElree, B., & Dosher, B. A. (1989). Serial position and set size in short-term memory. *Journal of Experimental Psychology: General*, *118*, 346–373.

Nieuwenhuis, S., & Monsell, S. (2002). Residual costs in task switching: testing the failure-to-engage hypothesis. *Psychonomic Bulletin & Review*, *9*, 86–92.

Oberauer, K. (2002). Access to information in working memory: Exploring the focus of attention. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *28*, 411–421.

Oberauer, K. (2003). Selective attention to elements in working memory. *Experimental Psychology*, *50*, 257–269.

Oberauer, K. (2006). Is the focus of attention in working memory expanded through practice? *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *32*, 197–214.

Philipp, A. M., & Koch, I. (2006). Task inhibition and task repetition in task switching. *European Journal of Cognitive Psychology*, *18*, 624–639.

Soetens, E. (1998). Localizing sequential effects in serial choice RT with the information reduction procedure. *Journal of Experimental Psychology: Human Perception and Performance*, *24*, 547–568.

Wagenaar, W. A. (1972). Generation of random sequences by human participants: A critical survey of literature. *Psychological Bulletin*, *77*, 65–72.